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Nutting: The New School of Animal Psychology

THE NEW SCHOOL OF ANIMAL PSYCHOLOGY.

C. C. NUTTING.

The title of this paper is in one sense a misnomer, from the fact that the prophets of the new school are inclined to deny any real psychology to animals. According to Morgan, mind is the wave of consciousness in its continuity.* Thorndyke says that "The mental stream is not continuous in animals."† If this is true, animals can not be said to have minds, and hence animal psychology can not exist.

However this may be, a discussion of those activities which have heretofore been regarded as psychical in animals forms the theme of a work embodying the views of the most radical of recent writers on comparative psychology.

This work is from the pen of Dr. Edward L. Thorndyke, fellow in psychology in Columbia University, and appeared in the form of a Monograph supplement in the *Psychological Review* of June, 1898. The views advanced therein are so iconoclastic that one rubs his eyes before realizing that these views are quite seriously advanced as the outcome of a great number of ingenious experiments reduced to the form of diagrams, time curves, etc.

The animals experimented with were cats, dogs and chicks, and they were taught to get out of variously contrived boxes under the stimulus of hunger, food being the invariable reward for success, and continued hunger the result of failure.

These boxes were contrived with undeniable ingenuity and were so constructed that the animal experimented with could escape by its own activity. The act of opening the box was of various degrees of complexity from a simple pressure to three separate movements, such as clawing, pushing and biting.

The hungry animal was placed in one of these boxes and the time in which it accomplished its exit was noted. As soon as escape was effected the animal was fed. This process

*Introduction to Comparative Psychology, p. 26.

†Animal Intelligence, p. 99.

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was repeated many times with the same animal, the time being always noted, until the appropriate act was thoroughly learned, and performed as soon as the animal found itself in the box. Then the progress of the education was platted in the form of a time curve for future study.

The following are the main conclusions drawn by Dr. Thorndyke from these experiments:

First.—The animals never thought about their situation at all, but out of a multitude of what might be called instinctive activities, such as clawing, pushing, etc., happened to hit upon the act that opened the door.

This successful act resulted in pleasure (*i. e.* food) and by repetition these pleasurable acts are "stamped in" and the proper association is formed through experience, while the unsuccessful acts are "stamped out" by the absence of pleasure.

It may be objected that the conclusion that the animal does not think about its situation at all is entirely gratuitous. It would not, in my opinion, be at all unreasonable to claim that the animal was doing a deal of thinking and that his thoughts might take some such shape as this. "This is unpleasant and I want to get out. I will try all sorts of ways, such as scratching, clawing and pushing until I find a way to escape." Shut a hungry small boy in a tight box and he would have numerous thoughts although he would probably act very much as the cat did, except that he would pound and kick and push instead of scratching and clawing and pushing.

Dr. Thorndyke is so impressed with the importance of his own conclusion that he says, "Surely every one must agree that no man now has a right to advance theories about what is in an animal's mind, or to deny previous theories unless he supports his theories by systematic and extended experiments." (Page 31.)

In other words the naturalist who may have spent the better part of a lifetime in carefully observing animals in a state of nature, must forever hold his peace in the presence of one who has put numerous cats in boxes, thus subjecting them to utterly unnatural conditions, conditions that would be more likely to inhibit than to encourage normal psychic acts.

Second.—Animals do not draw inferences, neither do they reason.

This conclusion is drawn from the fact that the animals give no evidence of observation of their surroundings, or of deliberation.

The author can hardly find words for his contempt for those who believe that animals reason. He says: "So, although it is in a way superfluous to give the *coup de grace* to the despised theory that animals reason, I think it is worth while to settle this question once for all." (Page 39.) Again he says: "I should claim that this quarrel ought now to be dropped for good and all * * * I should claim that the psychologist who studies dogs and cats in order to defend this 'reason' theory is on a level with the zoologist who should study fishes with a view to supporting the thesis that they possessed clawed digits." (Page 46.)

Third.—Animals do not imitate.—Finding that birds do imitate, he, very wisely, leaves them out of this discussion. The cases of imitation are "regarded as a specialization removed from the general course of mental development, just as the feathers or aortic arch of birds are particular specializations of no consequence for the physical development of mammals." (Page 47.)

The kind of specialization investigated by our author is illustrated by the man who, seeing another turn a faucet, turns a faucet himself to get a drink. In other words, "from an act witnessed he learns to do the act."

The experiments bearing on this question may be illustrated by the following:

A pen was so constructed that a chick could get out either by crawling under a wire screen or walking up an inclined plane. A chick who had learned to crawl under the screen was placed in this pen with an inexperienced chick. In nine minutes and twenty seconds the first chick crawled under nine times, and at the end of that time the other walked up the inclined plane and got out. "It was impossible to judge how many times the inexperienced chick actually saw the performance of the other."

Another inexperienced chick was tried in the same way and crawled under the wire in four minutes and twenty seconds, his companion having in the meantime crawled under four times. Now this would appear to be imitation, but no! The author says that probably he went under "not by imitation but by accident."

Here we have a clear case of "Heads I win, tails you lose." In the first experiment the inexperienced chick did not go under the screen, and in the second it did go under. It would have been manifestly impossible for that chick to give evidence of imitation.

Dr. Thorndyke admits that he can not insist upon these experiments as evidence against imitation.

Similar experiments with cats usually gave negative results. Every case in which imitation appeared to be present is explained away, ingeniously it is true, but not by any means in a manner convincing to the unbiased reader.

Dogs were experimented with, the results being always negative.

Dr. Thorndyke sums up the evidence regarding imitation as follows: "It seems sure from these experiments that the animals were unable to form an association leading to an act from having seen another animal or animals perform the act in a certain situation." "Not only do animals not have associations accompanied, more or less permeated and altered, by inference and judgment; they do not have associations of the sort which may be acquired from other animals by imitation." "But in any case the burden of proof would now seem to rest upon the adherents of imitation." (Page 62.)

Now it so happens that the present writer is in possession of such proof, and it is perfectly logical to claim that one positive case of imitation will justly outweigh any number of experiments with purely negative results. It happens, moreover, that the animal observed was a kitten.

This kitten was as wild as any that lives in the forest, and had the misfortune to fall into the brick flue through which cold air reaches the furnace in my house. The flue is about seven feet high, three feet wide and eighteen inches deep. From the bottom a double series of large tiles leads to a chamber beneath the furnace, in which there was at that time no fire.

Various attempts to capture the kitten resulted in its darting into the tiles. Efforts were made to prevent this by dropping a wire window screen in front of the openings to the tiles. These efforts failing, the screen was left leaning against the opposite wall of the flue. The mother of the kitten was then placed in the flue in the hope that she would carry her offspring, or induce it to follow her, through the tiles and out of

an opening from the chamber under the furnace to the cellar. This failed, probably because the kitten was unable to make a jump of about eighteen inches from the chamber to the opening into the cellar.

This latter opening was then closed so that the old cat would be forced to remain, and possibly nurse the kitten, which she refused to do, but jumped to the top of the screen and then out. The kitten attempted to follow her, climbed to the top of the screen, but could not jump the rest of the way.

Another screen was now placed on top of the first one so that the two together reached the top of the flue. The kitten very soon climbed nearly to the top, but was frightened and dropped down. She tried again and again and finally succeeded inmaking her escape. Now, although a careful watch had been kept she had not been seen to attempt to climb the first screen during the day and a half that it was in the flue before the mother was put in.

It might be thought that the kitten followed the mother by scent. But the mother *had not climbed the upper screen at all!* Neither did the kitten follow by sight, as it was several minutes after the escape of the mother that the second screen was introduced.

This appears to me to be as clear a case of imitation as could be conceived of, and I believe that anyone who has not prejudged the case will so regard it.

That Dr. Thorndyke has taken the position of an attorney for the prosecution of animals on the charge of being without mentality, is demonstrated by his treatment of the answers to a set of questions propounded to a number of professional animal trainers, five of whom, trainers of acknowledged reputation, responded. Four of the five believed that animals would learn through imitation, and one did not. This evidence not being to the liking of our author, was put out of court in the following language: "I cannot find that trainers make any practical use of imitation in teaching animals tricks*, and on the whole I think these replies leave the matter just where it was before. They are mere opinions—not records of observed facts." (Page 64.)

* See "The Nature of Animal Intelligence and the Methods of Investing it." Psychological Review, May, 1899, pp. 268-9, for Prof. Wesley Mills' discussion of this point.

I think that no injustice is done Dr. Thorndyke when I state candidly that his position, throughout the entire discussion, is not a judicial one. That he starts out to prove the thesis that the mentality of animals is much lower than heretofore supposed, and that this attitude constantly impairs the reliability of his conclusions.

After having denied the power of inference and of imitation, the author still further demolishes the work of his predecessors by the statement that "the ground-work of animal associations is not the association of ideas, but the association of idea or sense impression with impulse." (Page 71.)

Impulse is defined as "the consciousness a muscular innervation apart from that feeling of the act which comes from seeing one's self move, from feeling one's body in a different position, etc." Dr. Thorndyke does not believe that an animal can supply that impulse when it thinks of that act. For instance a cat can not go into a box by virtue of the thought of going in. It can not say to itself, "I will!" There must be the muscular innervation, accompanied by the consciousness that makes up the impulse. This matter, however, involves too elaborate a discussion to be followed here, interesting as it might be so to do.

Finally we come to the most astounding statement of all, which is the following:

"The possibility is that animals may have no images or memories at all, no ideas to associate. Perhaps the entire fact of association in animals is the presence of sense impressions with which are associated, by result and pleasure, certain impulses, and that therefore, and therefore only, a certain situation brings forth a certain act." (Page 73.)

The author believes that *acts* of recognition, for instance, may not be accompanied by any *feeling* of recognition at all.

We here arrive at as bald an automatism as could well be imagined. The following sentence will best convey Dr. Thorndyke's meaning: "A sense impression of me gets associated in my dog's mind with the impulses to jump on me, lick my hand and wag his tail, though he has not and never had any representation of me." (Page 74.)

Now it may be claimed that I have not done justice to the author under discussion because I have not given the arguments whereby he supports his theories. This course would, however, be impossible in the scope of this paper. I have tried to

give merely an idea of his methods and his conclusions. His monograph should be read by all who are interested in the subject of comparative psychology.

He is fairly entitled to much credit for his patience in devising and conducting experiments. In my opinion, however, this kind of experimentation is not the best method of solving the problems connected with the mentality of the lower animals, because it is certainly impaired by the unnaturalness of the whole procedure. Continuous handling, repeated confinement in boxes, and the pangs of hunger would surely and profundly affect the mental machinery of any person or animal.

The true method, it seems to me, is neither the piling up of anecdotes on the one hand, nor the cat and box method on the other, but careful, unbiased observation of animals that are not under pressure of excitement or hunger and are free to act on their own initiative. I would add, moreover, that they should, so far as possible, be ignorant of the fact that they are being watched.

To my mind the most serious criticism that can be made of the monograph under discussion is in reference to the attitude of its author toward previous writers and also toward his own work. It is not likely that the present generation of working naturalists, aside from the immediate friends of Dr. Thorndyke, will readily forgive his unconcealed contempt for such a man as Romanes, a man honored and loved by practically all his contemporaries and a naturalist who, in the minds of many, deserves to be classed among the foremost thinkers of his time.

Before quoting from Romanes, Dr. Thorndyke says: "These passages give an admirable illustration of an attitude of investigation which this research* will, I hope, render impossible for any scientist of the future." (Page 40.)

He sharply criticises the attitude of previous writers in the following words: "How can scientists who act like lawyers defending animals against the charge of having no power of rationality, be at the same time impartial judges on the bench?" (Page 4.)

Now I feel confident that no one, not a partisan of Dr. Thorndyke, can read his work without concluding that he clearly occupies the position of prosecuting attorney in this same case, and is, therefore, equally disqualified from acting

*Dr. Thorndyke here refers to his own work.

as judge. He rigidly excludes or at least minimizes every particle of evidence in favor of the accused.

In proof of this statement, witness his treatment of cats that do not come up, or rather down, to his expectations, and his naive brushing aside of the testimony of the animal trainers whose evidence is most damaging to his theories. For my own part, I still adhere to the belief that the argument submitted to this body in a former paper*, based on the multitude of homologies between man and the higher mammalia is a sound one, and that if this argument is to be overthrown it must be through careful observations of animals that are not psychologically disabled by starvation and imprisonment in boxes, however ingeniously contrived. And I further protest that the men who have gained their knowledge of animals by direct observation of animals in the field, have still their right to be heard on this question; that their observations demand consideration, and their opinions respect. In short, the old style field naturalist refuses to be ruled out of court by the experimental psychogolist of the new school. He emphatically denies jurisdiction, and appeals to the unbiased verdict of thoughtful men.

THE DISTRIBUTION OF FOREST TREES IN IOWA.

BY B. SHIMEK.

The discussion of the origin of our prairies, and of the distribution of our native forest trees, is as old as our knowledge of the central northwest. The earlier discussions were based on a knowledge of conditions as they existed when the white man first appeared in this section, and, though some of them are crude, and based upon insufficient observation, they fortunately give us at least a partial record of those conditions.

Later observers have the advantage of the results of a vast number of attempts at tree-planting, which have subjected existing conditions to a practical test, and which throw considerable light upon the causes which perpetuated the treeless prairies. From the very nature of the case, however, it is quite as difficult now to exactly distinguish in some cases

* "Do the Lower Animals Reason?" Proceedings Iowa Academy of Sciences, 1897